Towards a post-post-modern sociology of science

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Hillary Rose ends her very interesting account of feminist epistemology[1] complaining that "feminist theorizing is still not integral do sociology", and that "citation practices of both the old triumphalists and the new skeptics remain markedly gendered", meaning, I believe, that men still usually refer to men, and women, to women. She is obviously correct, as I can see by my own experience: most of the authors I read in the sociology of science literature are men (with some important exceptions, however, such as Karin Knorr, Helga Nowotny, Diana Crane), and I was very ignorant of the women authors she reviews. Should we take this fact as evidence that sexism is still alive in our field, or is there another explanation?

The most obvious difference between Knorr, Nowotny and Crane, on one hand, and Haraway, Harding, and the other authors discussed by Rose is that the former do not present themselves as feminists, and would probably not accept as a valid project the attempt of "re-vision a defensive feminist concept of objectivity", as Rose would put it. From this small fact, I can conclude that the problem is not with accepting women in the field, but with accepting feminist (of, for that matter, masculinist) epistemology.

I believe that Rose gives us the reason for this difficulty, which is that feminist epistemology, or sociology of science, is inescapably bounded to the "standpoint theory", and gets all the benefits, and suffers all the shortcomings, of such approach.

"Standpoint theory" is a new term for the theory developed by Georg Luckacs in his famous *History and Class Consciousness* (1920s'), which became the classic interpretation of Marxist epistemology, after its vulgarization in France by Lucien

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[1] **Hilary** Rose, *Epistemology in Action: contesting fortress masculinism in the sociology of scientific knowledge*.

Goldmann *(Sciences Humaines et Philosophie,* 1950s). In short, it said that workers, because they hold the future of mankind, can see better and further than the bourgeois; hence, worker's sociology, philosophy, economics (Luckacs restrained himself to the social sciences) are better than the conventional, bourgeois versions of each field. Not any worker, however, but only those that were conscious of their real interests, and active in the workers' organization, the Communist movement. This theory had a powerful influence among Marxist-oriented social scientists, but it finally fell in disrepute. First, because it served as a justification for the partidarization of the scientific field, making political and ideological criteria of demarcation to prevail upon those developed within each field of knowledge; second, because it granted legitimacy to the attempts of political leaders (and most notably Lenin and Stalin) to tell scientists what they should do, and evaluate their work; third, because it was based on a very simplistic theory of historical development, which was necessary to distinguish those that hold the keys to the future from those who don't. And finally, and probably more important, because it did not help to understand much of what was actually taking place in the most creative centers of scientific and academic production.

What remains from Marxist and Luckacs's epistemology is the acceptance of the notion that science is not isolated from social interests and motivations, but the product of social processes strongly shaped by them. In principle, it is possible and legitimate, for the sociologist of science, to identify the influence of cultural, gender, class and racial standpoints in different scientific constructions, and for the scientist to see if an explicit assumption of his own standpoint could not lead to a more satisfying understanding and interpretation of reality. It is impossible, however, to hold that one standpoint is better than others, since we don't know what the future will be like.

We all know the consequences and implications of this "multiple standpoint" perception of science: the shattering of the pretenses of universal validity, the blurring of the demarcation lines between scientific and other forms of knowledge, and the reintroduction of political and ideological arguments and criteria in the validation of scientific work. What these changes are doing to science, and more specially to the social sciences and the humanities, is something the sociologists of science have still to study and understand. It is clear that the social and intellectual boundaries of what one would call "scientific fields" are rapidly changing, but it is not clear that the final result would be the shattering of knowledge into a myriad of irreducible standpoints locked into endless power struggles. There are differences, but there are also communalities of interest, motivations and language; and nature (including social nature) is not as compliant to our interests and motivations as we sometimes wished.

In short, there is room, in the sociology of science, to study the implications, limitations and possibilities of knowledge associated with specific standpoints, but, because no standpoint is better than others, it is unlikely that these efforts would help us to foster the field of the sociology of science as a whole. The communality of interests and motivations provide the grounds for a mainstream of interpretation and theorizing - less ambitious and secure of itself than in the past, but still a mainstream. 1 believe that it is in this center, and not at its fringes, that the more interesting developments take place, including the working out of the full implications of race, gender, religion and other standpoint approaches to scientific knowledge.

Is Peter Wagner's *Sociology and contingency* an example of the emerging consensus, in these post-post-modernist years?[2] His paper shows how sociological certainty was based on three assumptions which seem to have disappeared in the last decades - that there was a place for a social science, better than other forms of knowledge; that society was organized in stable categories and classifications, that could be identified and measured; and that causal explanations were possible. Now we know the limitations of the social sciences, not just because we failed to become like the natural sciences, but because we know better how contingent the natural sciences are; we know that social classifications are always artificial, unstable and changing; and we are wary of strong causal explanations of human behavior. What happens, then, with the social sciences?

There are two possibilities, I believe. The first is to take a post-modernist stance, to give up social sciences as we used to understand it, and look for something else - hermeneutics, ethnomethodology, self-understanding, reflexiveness, and desconstructionism. The other is to try to see the place and role of the social sciences on the light of its uncertainties, and deprived of its epistemological and ontological certainties. If I understand Peter Wagner's text correctly, he sees these two possibilities very well, but at the end he remains post-modem, and chooses the first; "there is no sustainable theoretical argument nor any conclusive empirical evidence for any of the major terms of the modernist social sciences"; therefore, he makes a plea "for the search of a different language, one which avoids all categories that are inaccessible to ordinary reasoning altogether". This new language would accept that the world has structures, but unstable and changing structures, so unstable that "there is no way to predict the stage of this net [of linkages] at the next moment, since it cannot be known beforehand which linkages human beings will create or activate".

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[2] *Sociology and Contingency: Historicizing epistemology. or. why were sociologists in the 1960s so certain about their knowledge of the contemporary world?*

I have two criticisms of Peter Wagner's view. The first is that he tries to replace an ontology of certainty for an ontology of uncertainty, and, in doing so, he falls into an ontological trap I thought he would avoid. I don't believe in a world of fixed laws, but I don't believe in a world of total unpredictability and fully dependent on human agency either, as Peter Wagner does. Science (not only social science, but any science) is possible not because the world is ordained, but because we, as human beings, can ordain it, through our interaction with other people and with nature. You may not want to do it, but we may continue to do it, while understanding the limitations of our work. This is how I understand Bruno Latour's thesis that we were never "modem". Ontological certainty was always some kind of cover up for scientists, but sciences developed and strived even so, and there are no signs that they are disappearing.

The second criticism has to do with Wagner's preference for common language, and his plea against "categories that are inaccessible to ordinary reasoning". As Clifford Geertz showed years ago in his brilliant *Personal Knowledge*, "ordinary reasoning", or "common sense", is just a peculiar view of reality which is taken for granted by a given community, and does not have any special quality or truthfulness, except for its holders. "Ordinary thinking" for sociologists is very different from "ordinary thinking" for economists, and from "ordinary thinking" of French housewives. Which "ordinary thinking" should be the basis of Peter Wagner's new language? I imagine that his intention is to end with the demarcation between "scientific" and "non-scientific" language, as if the first were intrinsically superior to the latter, and I fully agree with this intention. But I would not like the engineers who service the airplane I came to this congress to replace their technical jargon and concepts by some other kind of "ordinary language", and the same holds for social concepts, in many instances of social life.

My own feeling is that the epistemological task of post-modernism, which was to challenge the arrogance of established science, is now fulfilled. A post-post-modern sociology of science (there has to be a better name!) should take into account the contingency of all knowledge, but should also incorporate the fact that technical knowledge, in the natural as well as in the social sciences, will continue to exist, and probably to expand, and not wither away in a sea of infinite standpoints and contingent languages. The days of the unassailed fortresses of arrogant knowledge are over, and scientific research is becoming much more pragmatic, contingent and aware of its limitations than before; but I cannot conceive of modem societies without their universities, research centers, social statistics and technical communities.

The next task is to go back to sociology as an instrument to describe and understand the roles of technical and specialized knowledge in societies that believe less and less in their ontological justifications, but use them more and more for all kinds of purposes. One of the most evident aspects of this situation is the growing gap between academic and other types of social sciences. While "modernist" social science is being demolished in the academy by the post modernist assault, empirically oriented and technically sophisticated social sciences are thriving in private firms, government departments, public opinion research companies, political parties, newspapers and TV companies, and census offices. It is about time for the social scientists to start looking around.